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RÉSUMÉ OF OBSERVATIONS CONCERNING THE SOLAR ECLIPSE OF MAY 29, 1919, AND THE EINSTEIN EFFECT[1]

1. A TOTAL eclipse of the sun is of more than passing interest, not merely to the astronomer but also to the geophysicist. Indeed, by reason of the supposed verification of the so-called Einstein effect during the solar eclipse of May 29, 1919, which, in consequence, may make that eclipse the most famous of all eclipses observed thus far, an eclipse of the sun has become of profound interest also to the physicist, to the mathematician, and to the philosopher, in general.

In the following brief account of the chief phenomena observed during the solar eclipse of May 29, 1919, the path of totality for which is shown in Fig. 1, the attempt will be made to bring out succinctly the various points of interest to men of science.

2. To give a personal touch let me first briefly state the results of my own expedition to Cape Palmas, Liberia, where totality was longer (6 minutes and 33 seconds) than at any other accessible station, where the sky was comparatively clear, contrary to all good meteorological predictions, and where totality

[1] Abstract of papers presented before the Philosophical Society of Washington (October 11, 1919 and January 3, 1920), Royal Astronomical Society of Canada, Toronto (December 2, 1919), American Academy of Arts and Sciences, Boston (January 14, 1920), American Philosophical Society, Philadelphia (February 6, 1920) and American Physical Society (New York, February 28). Also basis of public lectures delivered at the following universities: Toronto (December 2, 1919), College of the City of New York (December 4, 1919), Johns Hopkins (January 12), Yale (January 13), Brown (January 15), Columbia (January 16), Swarthmore (February 7) and Middletown Scientific Association of Wesleyan University (March 9).

TOTAL ECLIPSE OF MAY 28-29, 1919.

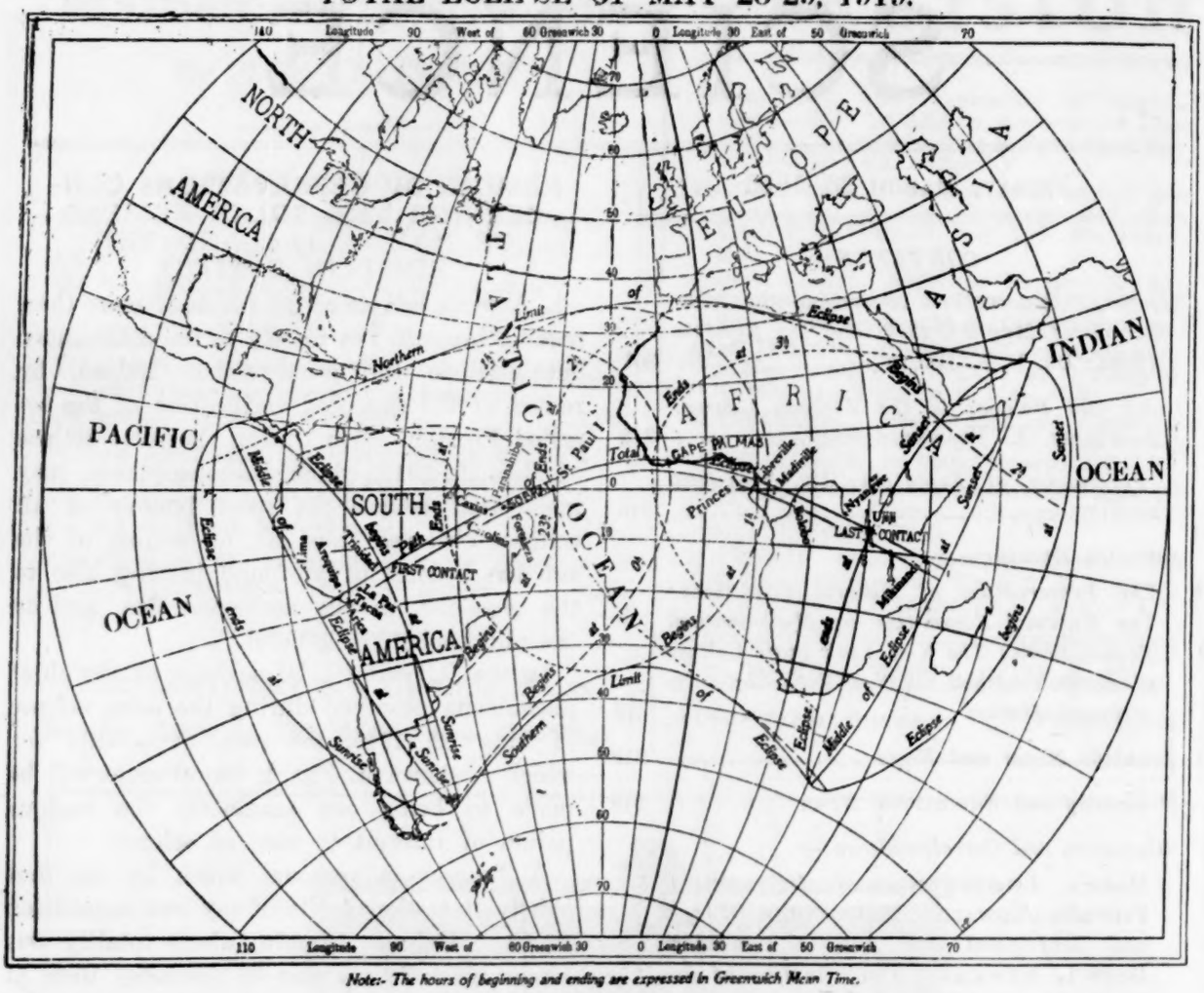

FIG. 1.

occurred at about one P.M. local mean time. The purpose of my expedition was not to make astronomical but *geophysical* observations, the chief of which were to be observations to detect, or verify, a possible effect on the earth's magnetic field such as has been shown by observations made under my direction, since the solar eclipse of May 28, 1900. Though it is not necessary for the detection of this magnetic effect to have a clear sky, as no layer of cloud could screen it, it has been my good fortune now three times[2] to have a clear sky when others whose work absolutely depended upon clear weather were not so fortunate.

3. When I left Washington early in March, 1919, it had been arranged that I should occupy conjointly with Dr. Abbot of the Smithsonian Institution, La Paz, Bolivia, in order that I might have there the conditions encountered during the eclipse of June 8, 1918, at my station, Corona, Colorado, the elevation of which is 12,000 feet. As Dr. Abbot intended to look after the photographic work, I did not provide myself with appliances for purely astronomical work. Upon arrival in England, it was found impracticable to reach a South American station in time for the eclipse; accordingly, it was

[2] Manua, Samoan Islands, April 28, 1911; Corona, Colorado, June 8, 1918; Cape Palmas, Liberia, May 29, 1919. In addition I made observations at Rocky Mount, North Carolina, of the total solar eclipse, May 28, 1900.

decided to proceed to Cape Palmas, Liberia, instead.

4. The station at Cape Palmas, Liberia, was one of five principal stations at which magnetic and allied observations were carried out by the Department of Terrestrial Magnetism of the Carnegie Institution of Washington in connection with the solar eclipse of May 29, 1919. Two of these stations were inside the belt of totality: Sobral, Brazil, in charge of Mr. D. M. Wise, assisted by Mr. A. Thomson; and Cape Palmas, in charge of the author, assisted by Mr. H. F. Johnson. A third station, at Huayao, Peru, north of the totality belt, was in charge of Dr. H. M. W. Edmonds; the fourth station, south of the belt of totality, at Puerto Deseado, Argentina, was assigned to Mr. A. Sterling; and the fifth, about 100 miles north of the belt of totality, at Campo Cameroun, was assigned to Mr. Frederick Brown. Observations were also made at a secondary station, Washington, outside the zone of visibility, by Mr. C. R. Duvall. In addition to these stations, special magnetic observations were made at the Department's magnetic observatory at Watheroo, Western Australia, and at observatories all over the globe, both inside and outside of the region of visibility of the eclipse, according to the department's program.[3] The reports already received from many of the foreign observatories indicate that the magnetic conditions were ideal for the detection of a possible magnetic effect. *There were clear indications at Cape Palmas of a magnetic effect in accordance with the results obtained during previous solar eclipses. Since Cape Palmas was nearly on the magnetic equator, the effect was especially noticeable in the vertical component of the earth's magnetic field intensity, or upon the magnetic dip.*

5. Our observation program at Cape Palmas (latitude, 4° 22′ N.; longitude, 7° 43′.7 or 30^m55^s West of Greenwich) included magnetic and electric observations, meteorological observations, shadow-band observations, times of contacts and photographs such as could be obtained with a small kodak camera. This comprehensive program was carried out successfully, excepting the atmospheric-electric work which, owing to the deterioration of the dry-cell batteries purchased in England, had to be abandoned. Sir Napier Shaw had kindly loaned us a Benndorf electrograph. Although I had stationed three observers, *no shadow-bands were observed this time, even greater precautions having been taken than at Corona during the eclipse of June 8, 1918, where they were observed.*

The full geophysical program, including complete atmospheric-electric observations, was carried out by our party in charge of Mr. Wise at *Sobral, where shadow-bands were clearly observed* by his assistant, Mr. Thomson.

6. *The eclipse of May 29 as observed at Palmas, was not nearly as dark, in spite of its long duration, as the much shorter one of June 8, 1918,* which I had observed at the mountain station, Corona, Colorado. There was a marked difference in light, both as seen visually and as shown by the photographs, between the inner corona and the outer extensions. The intense brightness of the inner corona may have been the cause of the fact that the eclipse of May 29, 1919, was not as dark as had been expected. Dr. A. C. D. Crommelin, the British astronomer at Sobral, Brazil says:[4] "The darkness during totality was not great; we estimated that the illumination was about the same as that 25 minutes before sunrise. The corona was very brilliant,

[3] The general scheme of work consisted in simultaneous magnetic observations of any or all of the elements every minute from May 29, 1919, 9^h58^m A.M. until 4^h32^m P.M., Greenwich civil mean time, thus for an interval of time from 35 minutes before the beginning until 48 minutes after the end of the eclipse on the earth. Similar observations for the same interval of time as on May 29 were to be made, if possible, on May 28 and 30 to afford the necessary means for determining the undisturbed course of the magnetic elements. Special continuous registrations were called for at magnetic observatories. Furthermore, special atmospheric-electric and meteorological observations were included in the program.

[4] *The Observatory*, London, October, 1919, pp. 370–371.

probably at least three times as bright as the full moon.[5]

7. The *large crimson prominence,* appearing at Cape Palmas on the southeast limb of the sun, turned out to be the largest prominence thus far photographed; it was a most conspicuous and startling object, projecting about 100,000 miles out from the sun's disk and having a base of 300,000 miles. On the southwest limb was a striking V-shaped rift in the solar corona which showed marked equatorial extensions to the west and east. The corona was approximately of the intermediate type between that which is seen during years of minimum sun-spot activity, when there are great equatorial extensions of the corona, and that shown during years of maximum sun-spot activity, when streamers of about the same length extend from the sun in every direction.

8. I succeeded in obtaining with my small camera, which is provided with an excellent lens, two sharp photographs of 10 and 20 seconds exposure, which when enlarged show well the chief features of the corona and of the prominence.[5] In addition, as the result of the interest aroused by a lecture which I was requested to give in the Methodist Church at Cape Palmas the day before the eclipse, a number of free-hand sketches of the corona were made for me by white merchants and by Americo-Liberians; these sketches, while not one of them is complete, show a number of interesting details.

9. The results of the *meteorological observations* at Cape Palmas will be of interest in connection with one of the theories suggested for the explanation of the bending of light rays, to which reference will be made later. Through the courtesy of Sir Napier Shaw and Colonel H. G. Lyons the British Meteorological Office loaned us a complete outfit of self-recording meteorological instruments, which were kept in operation by my assistant, Mr. Johnston, as long as the conditions permitted during our month's stay at Cape Palmas.[6] On the day of the eclipse there was a steady decrease in temperature from 12^h G.M.T., 0.7 minute after the first contact, to 12.7^h G.M.T., and then a more rapid decrease until the minimum temperature of 79°.4 F. was reached at 14^h G.M.T., which was approximately 0.4^h later than the middle time of totality. *The temperature drop during the time of the eclipse was, accordingly, about 2°.5 to 3°.0 F.* The increase in temperature after 14^h was rapid, the maximum 82°.7 F. being reached at 14.9^h G.M.T. The hygrogram for May 29 showed the following effect: the humidity, which was 71 per cent. at 12^h G.M.T. steadily increased to 78 per cent. at 14^h G.M.T. There was a more rapid decrease from 14^h G.M.T. to 15^h G.M.T., when the humidity was 66 per cent. *The maximum humidity, therefore, occurred at 14^h, or approximately 0.4 hour later than the middle time of totality. The barogram showed nothing marked during the time of the eclipse.*

At Sobral, Dr. Crommelin states:[7]

> The eclipse day opened very unpromisingly, the proportion of cloud at first contact being about 0.9. . . . The cloudiness during the early stages was doubtless the cause of the fall of temperature during totality being unexpectedly small; perhaps this latter fact was connected with the dead calm that prevailed during totality.

COMPLETE SERIES OF PHOTOGRAPHS

10. There was next shown in my lectures a complete series of photographs taken by the various observing parties, namely: C. G.

[5] During the duration of totality it was necessary for the author, (*a*) to take and record the readings of the magnetic-intensity variometer and attached thermometer at one- or two-minute intervals, and to check every fifth minute the Liberian assistant, Professor G. W. Hutchins, who had volunteered to take the declinometer-readings every minute; (*b*) to observe the times of contacts, obtain photographs, and give any required additional directions to the shadow-band observers. Thus though totality lasted at Cape Palmas 6½ minutes, it was none too long for a strenuous program in a tropic region.

[6] Mr. Johnston was also entrusted with the earth-inductor work.

[7] *The Observatory,* London, October, 1919, pp. 370–371.

Abbot, of The Smithsonian Institution, at La Paz, Bolivia; H. Morize, in charge of the Rio de Janeiro Observatory party at Sobral, Brazil; the British Astronomical Party (C. Davidson and A. C. D. Crommelin) at Sobral; L. A. Bauer at Cape Palmas, Liberia; and the British Astronomical Party (A. S. Eddington and Mr. Cottingham) at the Île of Principe in the Bight of Africa. Also slides of the great solar prominence of May 29, 1919, as photographed at the Yerkes Observatory, were exhibited. Grateful acknowledgement is here made to the Astronomer Royal of England, Sir Frank W. Dyson, and to those just mentioned, for copies of the photographs taken by their expeditions, as also to Dr. W. W. Campbell, who supplied slides showing how the corona changes its shape during the sun-spot cycle.

11. The chief features of the solar corona and prominence, as shown by the series of slides exhibited, have already been stated in paragraphs six and seven, where the observations at Cape Palmas were described. Careful measurements have been made between the various prominent features, as shown on the photographs taken along the belt of totality from Bolivia to the French Congo. From all the data supplied it is found that the mean heliographic latitude of the prominence during the time of the eclipse was about 18° south, and on the east limb, whereas the pronounced V-rift was about 45° south, and on the west limb. Practically diametrically opposite the V-rift was a less-pronounced rift, which I have called the U-rift. The solar prominence during the average time (11^h48^m G.M.T., civil) of totality at the two South American stations and the average time (13^h 55^m G.M.T., civil) of the two African stations changed comparatively little, though later in the day, according to the Yerkes Observatory photographs, kindly supplied by Professor Frost and Mr. E. Pettit, very great changes took place; thus, for example, at 20^h23^m G.MT., civil, the prominence had shot up to the height of 472,000 miles from the sun's limb.[8]

[8] See Mr. Edison Pettit's account in the *Astrophysical Journal*, for October, 1919, pp. 206–219.

12. A distinct purpose was had in mind in exhibiting first the various features of the solar corona and prominence, which persisted for four rotations of the sun and filled portions of the solar atmosphere with the products of eruptions, in order that one might be the better prepared to pass judgment upon the results concerning the deflection of light rays. For the same reason was given an account, though incomplete, of the results of our geophysical observations. We shall find that all the various phenomena though apparently unrelated have, indeed, an important bearing upon our next topic.

13. Altogether the solar eclipse of May 29, 1919, as observed at Cape Palmas, Liberia, was the most magnificent one of the four[9] it has been my good fortune to observe. Similarly Dr. Abbot with reference to what he saw at La Paz, Bolivia, says:[10]

> Taking into account the great length and beauty of the coronal streamers, the splendid crimson prominence throwing its glory over all, and the fact that the eclipse was observed so near sunrise from so great an elevation as 14,000 feet, with a snow-covered range of mountains upwards of 20,000 feet high as a background for the phenomenon, it seemed to the observers to be the grandest eclipse phenomenon which they had ever seen.

RESULTS OF OBSERVATIONS FOR DEFLECTION OF LIGHT

14. The most important result, undoubtedly, of the observations made by the astronomical parties during the solar eclipse of May 29, 1919, is the disclosing of the fact that the rays of light coming from stars, which appeared on photographs taken of the eclipsed sun and surrounding region, were bent by a measurable amount. No matter what the cause of the bending actually was, the fact is of profound interest and is bound to advance our knowledge. The chief possible causes which have been advanced thus far are:

(*a*) *Newton-Maxwell Effect.—Deflection of the rays of light by the sun's gravitational*

[9] See footnote 2.

[10] Abbot, C. G., and A. F. Moore: "Observations of the Total Solar Eclipse for May 29, 1919," Smithsonian Collections, Vol. 71, No. 5, p. 3, Washington, January 31, 1920.

action, just as the path of a projectile fired into the air is bent by the earth's gravitation pull upon the projectile, the amount of deflection being in accordance with Newtonian mechanics and Maxwell's electromagnetic theory of light. [If we assume, as did Newton, that light consists of corpuscles of matter traveling at great velocity, then it is easy to see why light should be bent under the action of gravity, for a cubic foot of light would in this case differ from a cubic foot of other ponderable material only in matter of *weight*. Newton in fact, had predicted such bending. But as our knowledge of light advanced we were forced to abandon Newton's theory for the undulatory or wave theory of light—a wave motion in the ether supposed to fill all space, the vibrations being electromagnetic ones according to our latest theory (Maxwell's). Light then consisting of some sort of wave motion possesses energy, or the power to do work, and it was furthermore shown about 20 years ago, by a Russian physicist, Lebedew, and by two American physicists, Nichols and Hull, that light exerts a measurable *pressure* when it falls upon a surface just as would material particles when fired at that surface. That light exerts pressure was in fact predicted by Maxwell a half century ago, but it was an open question whether light also had *weight*. The pressure of light resulted from the electromagnetic energy inherent in light, by which it is endowed with inertia just as is a body of material mass. Would gravity act upon something having electromagnetic inertia in the same way as upon a body of material mass? If so, the precise gravitational effect upon light could be predicted.] If a ray of light from a distant star just grazed the sun's edge (limb), it would be bent inwards (towards the sun) by 0″.44, as viewed by a solar observer. As the ray of light passed out of the sun's gravitational field on its journey to the earth it would suffer another deflection of about 0″.44, and in such a way that the final and total bending as perceived by an observer on the earth, would be away from the sun 0″.87—the angle which an object one inch high would subtend at a distance of three and three fourth miles.

(*b*) *Einstein Effect.—Twice the deflection of the rays of light predicted in* (*a*), this time *again by the sun's gravitational action, but according to the principles of Einstein's generalized relativity theory.* (These principles are tersely stated by Professor A. G. Webster):[11]

> First, that of the constancy of the velocity of light with respect to all directions and to any system moving with any velocity whatever with respect to any other system; second, a relation between time and distance such that either of two bodies seem shortened in the direction of their relative motion by an observer attached to the other; third, that it is impossible to distinguish a gravitational field from the acceleration of the frame of reference; and fourth, that everything that has mass, as determined by inertia, has mass of the sort determined by weight or attractability.

According to the Einstein law of gravitation, the deflection of a ray of light which grazed the sun's limb would be away from the sun by 1″.74[12], as we, or anyone outside the sun's gravitational field, might perceive it.

(*c*) *Refraction in the Solar Atmosphere.*—Bending of rays of light by refraction in passing through the sun's atmosphere, which, in more or less attenuated form, is known to extend out so far that the rays from all the stars concerned in the measurements would have to pass through it on their way to the earth.[13] [Such bending of light actually takes place all the time as the rays from the sun and other celestial bodies pass through our own atmosphere; the amount of atmospheric

[11] *The Review*, January 31, 1920, p. 116.

[12] See A. S. Eddington's "Report on the Relativity Theory of Gravitation," London, 1920, p. 55.

[13] See Dr. H. F. Newall's suggestive note in *Monthly Notices of the Royal Astronomical Society*, Vol. LXXX., No. 1, November, 1919. Mr. Jonck-heere (*The Observatory* for August, 1919, Vol. XLI., p. 216) suggested that refractions may be caused by "a hypothetical condensation of ether near the sun." This hypothesis is treated by L. Silberstein in connection with the theory of Stokes-Planck's ether in the *Phil. Mag.*, Vol. 39, pp. 161–170, February, 1920.

refraction of light depends upon the atmospheric conditions (temperature, pressure, humidity) and decreases with altitude of the celestial body above the horizon. Adequate correction of the observed deflections because of this known source of bending in the earth's atmosphere had to be made.]

(*d*) *Terrestrial Refraction Effects.*—Disturbance refraction effects as rays of light from the distant stars passed through the region of the earth's atmosphere affected by the solar eclipse, especially during totality. This cause would give a deflection in the right direction but apparently not of sufficient magnitude to account for the observed effects.[14]

15. The law of decrease in the amount of deflection of light for causes (*a*) and (*b*) is a very simple one, namely, inversely as the distance of the ray from the sun's center when it passes through the solar gravitational field. For cause (*c*) the law may or may not be as simple as that just stated, depending among other things on the variation of the density and distribution of the solar atmosphere with distance from the sun.[15] For our own atmosphere the law of atmospheric refraction is a somewhat complicated one. Sufficient has been said to show how intensely interesting a full discussion of the observed deflections of light will prove to be. Even had no deflections been observed a valuable contribution to science would have resulted.

16. Table I. contains the deflections of light rays observed by the British Astronomical Expedition, equipped and sent to Sobral, Brazil, under the direction of the Astronomer Royal of England, Sir Frank W. Dyson. Let α be the total deflection of a light ray coming from a star, S, and passing through the sun's gravitational field and finally reaching the observer on the earth. Suppose α_0 be the value of α if the ray grazed the sun's limb, and ρ, the radius vector or distance from the sun's center to the ray of light passing the sun. (The values of ρ for the various stars are given in units of the sun's radius in the third column of the table.) Then

$$\alpha = \alpha_0/\rho. \qquad (1)$$

As already stated, according to the Newton-Maxwell law, $\alpha_0 = 0''.87$, and according to the Einstein law, $\alpha_0 = 1''.74$. As the observed effects appear to agree better with the Einstein law, the comparison is made in the table with those computed from that law. The main tabular quantities have already been given in various publications. Detailed data were also courteously furnished by the Astronomer Royal for my lectures; these data gave the results separately for each of the seven stars and for each of the seven plates obtained by the observer, Dr. A. C. D. Crommelin, using a 4-inch lens of 19-foot focus and an 8-inch cœlostat. From the detailed data members of my staff computed the probable errors found in the last three columns of the table. From the coordinates furnished we also were able to compute the angle A, which the radius vector, ρ, to any star made with the declination axis, counting it from the north end in the direction east or west; these values are contained in the fifth column. The computed effects in right ascension and declination were obtained by multiplying the value of α from (1) by $\sin A$ and $\cos A$, respectively. From the fourth column it will be seen that the photographic magnitudes of the stars ranged from 4.5 to 6.0. The British astronomers were thus exceedingly fortunate in being able to make their observations during a solar eclipse when there was an exceptionally rich field of bright stars, the Hyades, close to the sun.

17. It will be observed that from the figures

[14] This hypothesis was suggested by Dr. J. Satterly at the close of the author's lecture at the University of Toronto, December 2, 1919. It had also occurred to Dr. Alexander Anderson, of the University College, Galway, and has been discussed by him and others (Eddington, Cromelin, Cave, Dines and Schuster) in various issues of *Nature*, December 4, 1919–January 29, 1920.

[15] In the discussion of the author's paper before the American Academy of Arts and Sciences, January 14, 1920, Dr. E. B. Wilson, of the Massachusetts Institute of Technology, suggested that if the density of the solar atmosphere varied inversely as the square of the distance from the sun's center, a refraction law would result similar to the one for causes (*a*) and (*b*).

in the three columns headed O–E (Observed-Einstein value that), relatively. the observed right-ascension deflections depart more markedly from the computed ones than do the observed declinations-deflections. The observed total deflections in every case, except for star 11, exceed the Einstein values.

nomical Expedition, at the Ile of Principe, west coast of Africa, where the weather conditions were unfortunately not as favorable as at Sobral, showed only a few stars and the scale could not be directly determined as it was not possible to remain at Principe the required time. Instead, plates of another region

TABLE I

Comparison of Deflections of Light Rays Observed by the British Astronomical Expedition at Sobral, Brazil, May 29, 1919, with Values Computed according to the Einstein Theory

(*Instruments:* 4-inch lens of 19-foot focus and 8-inch coelost at. *Observer:* A. C. D. Crommelin)

No.	Star	Dist. in Sun's Radii	Phot. Mag.	Angle A	Right Ascension			Declination			Total			Probable Error		
					Obs'd	Einstein	$O-E$	Obs'd	Einstein	$O-E$	Obs'd	Einstein	$O-E$	R. A.	Dec.	Tot.
				°	″	″	″	″	″	″	″	″	″	″	″	″
3	κ_2 Tauri	1.99	5.5	8.2W	−0.20	−0.12	−0.08	+1.00	+0.87	+0.13	1.02	0.88	+0.14	.02	.02	.02
2	Pi. IV. 82	2.04	5.8	96.2E	+0.95	+0.85	+0.10	−0.27	−0.09	−0.18	0.99	0.86	+0.13	.04	.05	.04
4	κ_1 Tauri	2.35	4.5	8.6W	−0.11	−0.10	−0.01	+0.83	+0.74	+0.09	0.84	0.75	+0.09	.03	.03	.03
5	Pi. IV. 61	3.27	6.0	144.8W	−0.29	−0.31	+0.02	−0.46	−0.43	−0.03	0.54	0.53	+0.01	.04	.05	.05
6	υ Tauri	4.34	4.5	6.3E	−0.10	+0.04	−0.14	+0.57	+0.40	+0.17	0.58	0.40	+0.18	.04	.04	.04
10	72 Tauri	5.19	5.5	14.9E	−0.08	+0.09	−0.17	+0.35	+0.32	+0.03	0.35	0.34	+0.01	.04	.05	.05
11	56 Tauri	5.38	5.5	86.6W	−0.19	−0.32	+0.13	+0.17	+0.02	+0.15	0.25	0.32	−0.07	.06	.02	.05

18. From the observational results in Table I., the resulting value of the deflection, α_0, at the sun's limb, as published by Dr. Crommelin, is 1″.98,[16] thus agreeing with the Einstein predicted value, 1″.74, within 14 per cent. The result from the astrographic plates taken by the other British observer at Sobral, Mr. C. Davidson, using the astrographic object glass of the Greenwich Observatory in conjunction with a 16-inch cœlostat, was not so satisfactory, the star-images being diffuse on account of a probable change in figure of the cœlostat mirror; the discordance between the mean results from the individual plates was said to be rather large, but from the whole series an outward deflection reduced to the limb, of 0″.93, or 0″.99, according to the method of treatment, was found, with a probable error of about 0″.3.[16]

19. The plates taken by Dr. A. S. Eddington and Mr. Cottingham, the second British Astronomical Expedition, at the Ile of Principe, west coast of Africa, where the weather conditions were unfortunately not as favorable as at Sobral, showed only a few stars and the scale could not be directly determined as it was not possible to remain at Principe the required time. Instead, plates of another region of the sky taken at the same altitude were used and compared with plates of the same region and of the eclipse-field obtained previously at Oxford. The determination of scale was therefore somewhat weak, though the uniformity of temperature at Principe was in its favor. The final result of the discussion of the plates gave an outward deflection of 1″.61 with a probable error of 0″.3.[17]

20. *Except then for the unsatisfactory Sobral astrographic plates, the general conclusion to be drawn is that deflections of light were observed by the British astronomers that agree better with the Einstein law of gravitation (Cause b) than with the Newton-Maxwell law (Cause a).* This is well shown by Fig. 2, constructed by the Department of Terrestrial Magnetism, giving a graphical representation of the law of variation with distance followed by the observed deflections for each star, as well as by the computed ones on the basis of causes *a* and *b*. It is seen at once that, excepting the most distant star (56 Tauri), each star shows a deflection agreeing better with the Einstein value than with the Newton-Maxwell

[16] See *Nature*, November 13, 1919, p. 281. The probable error as given by Dr. Crommelin is 0″.12, whereas Dr. H. Spencer Jones, of the Greenwich Observatory, in his summary (*Science Progress*, January, 1920, p. 372) gives 0″.06.

[17] See reference to Dr. Jones's article in previous footnote.

one. Though the result from 56 Tauri is discordant, it still is about midway between the two computed curves (Causes *a* and *b*). It should be noted also that the probable error of observation, as shown by the size of the circle around each star, is largest for 56 Tauri, so

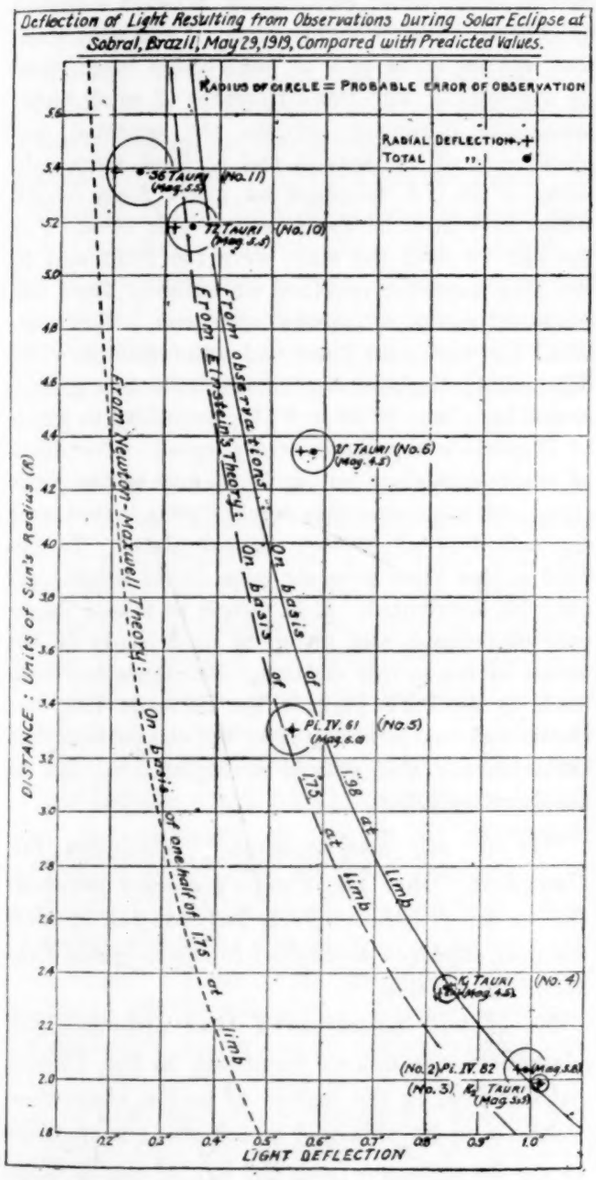

FIG. 2.

that no wholly safe inference as to cause of its departure from the Einstein value may be made.

In view of the recognized difficulties of the observations and the conditions under which they had to be made, and recalling, furthermore, that the preparations and securing of the requisite instrumental equipments were undertaken during the stress of the great war, every one will surely agree that the Astronomer Royal of England and the British observers are heartily to be congratulated upon the splendid results of their labors.

ANALYSIS OF OBSERVED LIGHT DEFLECTIONS

21. In conclusion an analysis was sketched of the observed light deflections and some evidences were pointed out showing that while the simple law (1) was followed to the greater extent, the effects in addition to varying inversely as the distance from the sun's center also apparently depended in some measure upon the heliographic latitude, ϕ, of the star. As a consequence the observed effects are not strictly radial, the departures from radiality occurring in a strikingly systematic manner, and not in the accidental manner that would be the case if the non-radial effects were attributable wholly to errors of observations. When such trigonometric functions are added to law (1) as would arise from forces similar in effect to centrifugal ones, the additional effects are largely accounted for. This possible additional cause, whatever it turns out to be, is designated as *e*. In complete allowance for differential atmospheric refraction effects in the earth's atmosphere may also be the cause of non-radial effects. Resolving the observed actual deflections into two components, radial (along radius vector) and the other non-radial (perpendicular to radius vector), preliminary computations were made with the aid of the expanded law.

$$\alpha = \frac{\alpha_0}{\rho} + f(\rho, \phi). \qquad (2)$$

A value resulted for α_0 agreeing better with the Einstein value of 1".74, than the value 1".98 stated in paragraph 18. A future paper will give further account of this interesting matter.[18] I must not fail to record here

[18] The possibility of non-radial effects arising from cause *e* was announced at the meeting of the American Philosophical Society, Philadelphia, Feb-

the assistance received in the construction of diagrams and in the computational work from members of my staff, viz., W. J. Peters, H. B. Hedrick, C. R. Duwall and C. C. Ennis.

22. It is, of course, impossible without further analysis to state at present just what portion of the observed effects may be accounted for by the various causes described in paragraphs 14 and 21. Dr. Newall, for example, see reference in footnote 13, is ready to accept an effect from cause *a* (the Newton-Maxwell effect), but prefers considering the possibility of accounting for the greater portion of the remaining effect by cause *c* (Refraction in the Solar Atmosphere).

23. If it should prove to be the case that the observed light deflections are the result of a combination of the causes mentioned, the way may be open to explain the *results obtained by Dr. W. W. Campbell's eclipse expedition of June 8, 1918, at Goldendale, Washington.* Using two 4-inch photographic objectives photographs were taken of the sun and its surroundings, the exposures being 110 seconds, 50 stars to the ninth magnitude being recorded. He states his results as follows:[19]

> The measurement of photographs, 14 inch × 17 inch in size, is a difficult problem even with suitable apparatus: we found it necessary to construct a special measuring machine, and this was made in our own shops. Duplicate photographs of the eclipse field were secured at Mount Hamilton seven months after the eclipse. As the difference of latitude between Mount Hamilton and the eclipse station is only a few degrees, no errors were introduced by not obtaining the comparison field at the eclipse station. These were taken at the proper altitude to avoid the chief refraction troubles in the comparison with the eclipse plates, so that second differences of differential refraction alone entered into the comparison. The plates were measured right and left. The same scale-divisions were used for corresponding pairs of stars. As far as possible the measures were freed from any known source of error. The corrected differences of position were measured along radii from the sun to each star and were arranged in order of distance from sun to star. Dr. Curtis was not able to say that there was anything systematic about these differences, which showed no change of the order required by Einstein's second hypothesis. The probable error of one star position was the order of 0″.5, regrettably large when we are dealing with the differences of small quantities—the difference between the expected displacements of the nearest and furthest stars only being 0″.26. A telescope of great focal length would have been of great help in this work. For the one we used the stars were too faint and in the long exposure required we suffered from the increased extent of coronal structure. Curtis divided his stars into inner and outer groups. The differential displacement between the two groups should have been 0″.08 or 0″.15, according to which of Einstein's hypotheses was adopted. The mean of the results came out at 0″.05 and of the right sign. After getting this result Curtis looked over the collection of 40-foot coronal plates. In the 1900 eclipse there were six stars fairly bright, but not well distributed. It is useless to take a duplicate photograph now owing to uncertainty in the values of the proper motions. Reference has been made to the Paris plate in the Carte du Ciel, but Curtis was unable to say from the comparison that the innermost star showed a displacement due to the Einstein effect.

"It is my own opinion," concludes Dr. Campbell, *"that Dr. Curtis's results preclude the larger Einstein effect, but not the smaller amount expected according to the original Einstein hypothesis."*

24. It will be observed that although Dr. Campbell was not so fortunate as the British astronomers in the matter of bright stars close to the sun, he obtained an effect at more than twice the distance from the sun of the farthest star (56 Tauri), shown in Fig. 2, in the right direction and of about the same amount as that given by cause *a* (Newton-Maxwell Effect). It is of interest to note here that the farthest star, 56 Tauri, in Fig. 2, also gave a deflection approaching that given by cause *a*, though since that star gave the largest probable error, not much weight is to be attached

ruary 6, 1920, and slides were shown exhibiting the systematic character of these effects. The matter was gone into more fully at the New York meeting of the American Physical Society, February 28, 1920.

[19] *The Observatory*, London, Vol. XLII., No. 542, August, 1919 (298–300).

to the fact. It would be of great importance to know, of course, whether as the distance of a star from the sun greatly increases, the deflections of light will correspond more and more closely with that given by cause *a*. There is no possibility that the Einstein effect with increased distance will merge into the Newton-Maxwell effect, since theoretically the former should always be twice the latter. However, if the main cause of light deflections should prove to be *a*, *c* and *e*, or *a* and *e*, or similar ones in effect, it may be possible, as already stated, to harmonize Dr. Campbell's results with those of the British observers. As a caution it may be well to bear in mind that Dr. Campbell unfortunately was obliged to get his results from very distant stars and hence had to look for quantities very much smaller than those concerned in the British observations of the solar eclipse of May 29, 1919.

OUTSTANDING MOTION OF MERCURY'S

25. As a further proof of the Einstein theory of gravitation has been cited the very satisfactory way[20] in which the theory accounts for the outstanding motion of the perihelion of mercury, characterized by the late Professor Simon Newcomb as one of the greatest of astronomical puzzles. Dr. Charles L. Poor, of Columbia University, at the close of my lecture there on January 16 suggested that the outstanding motion of Mercury's perihelion could also be fully accounted for if the equatorial radius of the sun were found to exceed the polar radius by 0″.5, so that the sun would not be truly spherical. Seeliger advanced the hypothesis[21] "that the scattered zodiacal-light materials, if condensed into one body might have a mass fairly comparable to that of the little planet Mercury," and he has concluded that the attractions of the zodiacal light materials upon the planet Mercury could explain the deviation of that planet from its computed orbit. This problem can not yet be regarded as definitely settled."

EINSTEIN DISPLACEMENT OF LINES OF SPECTRUM

26. Dr. Einstein appears to regard as essential to this theory the verification of the shifting towards the red of the lines of the spectrum of light from the sun and stars. However, Sir Joseph Larmor, according to a paper presented before the Royal Society on November 20, 1919, does not apparently agree with him. The predicted effect has not yet been successfully observed, or, as Professor Joseph S. Ames in his concluding remarks at the end of my lecture at the Johns Hopkins University put it, "has not yet been disentangled from the various possible other causes for shifts of the spectrum lines."

CONCLUDING REMARKS

27. The endeavor has been to set forth impartially all the facts pro and con with reference to the question of the verification of the Einstein theory of gravitation by the recent astronomical observations, so as to enable the reader to form an independent judgment and reach his own decision. Though we may differ as to whether the Einstein theory has been definitely verified, or not, one result of fundamental importance appears to have been established with fair certainty, upon which perhaps chief emphasis should be laid, viz.: that *light has weight*—just how much depends upon whether the Newtonian or the Einstein principles will ultimately be found correct. Possibly the best attitude to take is that of open-mindedness and to let no opportunity pass by for further experimental tests. The British astronomers are already zealously preparing to make observations during the solar eclipse of September, 1922, which will occur in Australia. Perhaps one of the most satisfactory results of the discussion aroused by the subject has been the stimulus imparted to further research in many fields, which is bound to bear fruit.

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[20] See A. S. Eddington's *Report on The Relativity Theory of Gravitation*, London, 1920, p. 52.

[21] W. W. Campbell, "The Solar System," published in *The Adolfo Stahl Lectures*, p. 10, San Francisco, 1919.

UNITY AND BALANCE IN THE ZOOLOGY COURSE

In an earlier number of this journal,[1] apropos of an article by Professor Bradley M. Davis upon the botany course of the future, I briefly described the introductory course in zoology in operation for several years at the University of Michigan, and pointed out some of the advantages which a course centered around biological principles possessed over the usual course based on the dissection of types. Many inquiries concerning this course were received from all over this country, and several from the other side of the world, indicating a feeling of unrest and dissatisfaction with the present prevailing type course. Some of the writers of these letters clearly recognized the defects of the present method of teaching, and had striven to remedy them without completely reorganizing their courses. Others, while perceiving that something was wrong, had failed, it seems to me, to discern wherein lay the difficulties. In the hope that a clear understanding of the fundamental mistakes of the type course will assist in removing these difficulties, I have undertaken to present herewith what appear to me to be the requisites of the beginning course.

The nature of the first course in science should not be a matter of untrammeled opinion, it should be determined by certain principles. If those principles can be agreed upon, the details may perhaps be varied without harm. I submit two propositions which I regard as almost axiomatic, namely, that the course should be representative, and that it should possess unity. If these propositions are valid, the remainder of this article may have some value.

To apply the first of these rules, it is necessary to have in mind the content of the subject. On this question there may be differences of opinion, but most of these opinions can probably be arranged into two fairly well-defined groups. Zoology consists either (1) of a knowledge of Protozoa, Porifera, Coelenterata, Platyhelminthes, etc., or (2) of a body of principles that may be brought under such rubrics as morphology, physiology, ecology, taxonomy, geographical distribution, paleontology, and evolution. Between these views the teacher must make a choice, if he is to make his course representative, and the nature of the course will depend upon his decision. If the first of these views of the content of zoology should prevail, he who studies cell permeability in *Paramecium* is to be regarded as a protozoologist, not as a physiologist, or else he is not a zoologist at all; the student of heredity in *Drosophila* is a dipterist, not a geneticist; and one who traces the origin of the horse is a mammalogist, not a paleontologist or evolutionist. Very few of the scholars mentioned would be content with the proposed appellation.

If the second conception of the content of zoology be entertained, as has been done in the preparation of our first course, the incongruities just referred to disappear. Other difficulties are also removed, for the seven divisions of zoology named above are not mutually exclusive, but overlap, a circumstance which, far from being a misfortune, is of much value in connection with the second proposition to be developed later. Genetics might fairly be added as an eight division, but its main features are either morphological, or physiological, or evolutionary.

The beginning course must contain the elements of each of these branches of the subject, if it is to be a *general* course. Whether the course should be general or not may be debated, but if it is to be general it must include something from each field.

The classical course in zoology is morphological, a dissection of types of the chief animal groups. Very little even of physiology has been included in it, until in recent years in a very few institutions. Such a course was the proper course once upon a time, when zoology was an almost purely morphological subject. But as the subject grew, the type course became a misfit. It has been a misfit for a long time.

Good teachers have attempted to ameliorate this growing inaptness of their courses by

[1] Science, December 27, 1918.

putting the non-morphological phases of zoology into their lectures and recitations. But the laboratory work has inevitably put an over-emphasis on the morphological side, and may even have over-emphasized the physiological. The seven branches of the science need not, of course, be treated equally. Morphology deserves a greater share than any of the others, for each of the divisions is partly morphological. But a course on morphology alone (or nearly alone) can scarcely be representative. Unprotesting use of the type course means either that the teacher regards the content of zoology as Protozoa, Porifera, Cœlenterata, etc., or that he is satisfied to administer an unbalanced ration to his students.

Quite independent of the foregoing consideration of the content of zoology is the question of unity of the first course. Whether the type course or the topic course be employed, that course should be unified. It should proceed step by step, one thing leading up to and necessarily following others. Unity has not been ignored by those who employ the type method, but they have justified their course by the evolutionary series which the animal scale is supposed to present. When the animal series was thought to be single and continuous, that was a fair assumption. But this notion of the phylogenetic tree has been largely abandoned, it is recognized that the animal series is a disjointed one. At least if there are connections everywhere, they are so attenuated in places that even a superior student is unable to detect them. The step from an echinoderm to an annelid is not an easy one, nor the step from a mollusk to an arthropod.

The lack of unity consequent upon the employment of type dissections has long been recognized, and has led to the widespread notion, referred to above, that something is wrong with the beginning courses in biology. One can not converse long with teachers of biology who are interested in the pedagogy of their work, without encountering the question, what is to be done about the beginning course? Sometimes the unrest is vague, sometimes it is not recognized that lack of unity is the fundamental defect, but in few quarters is the present course regarded as satisfactory.

Various proposals have been made for remedying the defect. One plan offered by a botanist for the beginning course in botany is frankly to make the course practical, utilitarian. Since there may readily be a counterpart of this plan on the zoological side, it is worth considering. The author of this proposal does not recognize lack of unity as the thing to be overcome. He would, for example, study wheat: where it is grown, the proper kinds of soil, its uses, its markets, etc.; then potatoes, their soils, geography, industrial uses, diseases and so on. However desirable a course in agriculture may be, little can be said for the above plan with regard to its unity. One plant may, it is true, unify soils and markets after a fashion, but the gap between wheat and potatoes can hardly be bridged in the same arbitrary manner. The proposed course is simply a type course of another kind, the types being no more closely connected than are the taxonomic groups of organisms to which they belong.

One experienced teacher of zoology proposes that the history of the development of the biological sciences be employed. This teacher has detected the fundamental defect of the present course, and his plan is avowedly an attempt to secure unity. His plan could be successful if the historical development of the science were steadily from the simple to the related complex. If one could learn the history of the rise of a subject by the same steps as he learned the content of the subject, then history would be a unifying study. But were that done in zoology, one would study the development of the chick before he learned of the existence of cells: and he would know of the parthenogenesis of the honey bee before he knew the existence of germ cells. Whereas theoretically simple things should be discovered before complex ones, many circumstances, such as the lack of microscopes, has prevented that order from being followed.

Are we to forget that we now have microscopes, in order to let history unify our subject for us? History may explain a good many discrepancies, especially in earlier biology, but it does not unify anything. History unifies only subjects that are essentially historical in their nature, like political development, or philology. I do not mean that history is uninteresting or unimportant, for it is neither; but it unifies only the history, not the *content*, of biology. Only the facts of a science can unify the science itself.

Unity can be acquired only by arranging subjects, placing the simple first, and laying thereby a foundation for related subjects that are more complex. Each subject should lead to another, and rest upon those that precede. Such unity a course based on the dissection of types can have only in small degree. Otherwise one teacher could not begin with Protozoa, another with vertebrates, or another with Arthropoda which are followed by Protozoa, leaving the vertebrates to the last. Did types insure unity, we would not have that interesting chapter on "animals of uncertain affinities" squarely in the middle of the course. Nematodes do not lead naturally to the Bryozoa, nor do the annelids obviously follow the echinoderms. There is no manifest necessity for having the mollusks precede the arthropods. The teacher of the type course may claim unity for his course, on the ground that he goes from the simple to the complex. A grindstone, a bicycle, a typewriter and a calculating-machine may be arranged in order of complexity, but the unity permeating the series still not be very obvious.

Homology, on the contrary, does lead to taxonomy, taxonomy and ecology to distribution, distribution in space to distribution in time. Cell division leads to cell aggregation, and reproduction to embryology. The connections stated are not merely obvious, they are necessary.

The study of topics entails certain difficulties, one of them being the larger amount of diverse material required in the laboratory. Some may think that this use of many different animals is confusing, rather than unifying. Our experience indicates that such is not the case. Using many animals to demonstrate the truth of the cell doctrine is not more confusing than the study of profit and loss in arithmetic by problems involving vinegar, woolen goods, automobiles, and ostrich feathers. What would be thought of an arithmetic that employed problems relating to vinegar for addition, division, profit and loss, compound interest and cube root, before woolen goods were used to illustrate the same operations? Or what of a school system in which vinegar was studied chemically, biologically, and industrially before woolen goods were studied from the same points of view? Those would be type studies, type arithmetics, type school systems.

In only one other science, so far as I am aware, do teachers as consistently use the type method as we have done. Whether another method would do as well in that subject I am not qualified to say. Biology is, then, one of the few sciences which have allowed their wealth of material to obscure their subject matter.

How do the students react to the treatment I have described? Perhaps, although the course has been given seven times, we have not been using the new method long enough to speak authoritatively; but some things seem to be observable. I have seldom heard students ask that question formerly not infrequently heard, not only in our own laboratories but in those of other institutions, "How much of all this are we expected to remember?" Students now recognize for themselves that the things which they study are important, for they draw conclusions from them. They have perhaps been quicker than teachers to see the advantages of the new method. Verily, these things were hid from the wise and prudent, and were revealed unto babes.

If culture be measured by the number of ways one has of entertaining himself, certainly the knowledge of biological principles far outweighs from the cultural standpoint

an acquaintance with the details of structure of selected forms. For a knowledge of animals, as members of taxonomic groups, is not lacking in those who pursue zoology in the way I have outlined; and about these animals there is always something besides structure that is worth knowing. In order that these worth-while things may be known adequately, they must be the subject matter of the laboratory exercises as well as the recitations.

Nothing in this article is intended to imply that advanced courses should be of the kind described for beginning students. It is recognized that to become a zoologist, or to prepare for certain professions, it is necessary to have a systematic knowledge, not only of taxonomic groups, but of several other fields of zoology as well. In the acquisition of such knowledge there must be courses in which facts seem to outweigh principles. But to attempt to gain such knowledge in the elementary courses, even for those who must later acquire it, is neither necessary nor desirable.

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A FORERUNNER OF EVOLUTION

BICENTENARY OF CHARLES DE BONNET, NATURALIST AND PHILOSOPHER

MARCH 13, 1920 marks the two hundredth anniversary of the birth of one of the most interesting of eighteenth century scientists, whose researches in entomology and botany were of solid and permanent importance in the history of these branches of learning, and whose philosophy, if superseded, was at least interesting and to some extent prophetic; yet who is comparatively seldom spoken of to-day.

Charles de Bonnet on that date was born in Geneva, the sometime home of one against whom he wielded most fiercely his philosophic pen—Jean Jacques Rousseau. Rather curiously, de Bonnet's birth and death dates anticipate by an exact century those of a pioneer of evolutionary science, John Tyndall. The earlier master died on May 20, 1793, after a life almost uneventful except for its mental activities.

One of the most striking facts about de Bonnet's career is the extreme precocity of his talent. His entire work in natural history is crowded into the first twenty-five years of his life; after which failing eyesight, induced by close work with the imperfect microscopes of the day, turned him perforce from laboratory research to theoretical speculation.

At sixteen he read Réamur's work on "Insectology." It proved the turning-point of his life. Born of a Huguenot exile family, all of whom were accustomed to hold high offices in the Swiss government, de Bonnet was studying law with the expectation of following in the footsteps of his kinfolk. His introduction to entomology ended his interest in law; although he persevered in his studies until he attained the degree of Doctor of Laws, he never practised, but devoted the rest of his life to the science which had become his passion.

Two years after he first read Réaumer and Pluche, he sent to the former a long list of "additions" to his works, based on further investigations. What was Réaumur's astonishment to discover that his valuable collaborator was a boy of eighteen! By the time he was twenty, de Bonnet had established the fact of at least usual, and probably invariable, parthenogenesis in aphides. Before he was of age, he had been appointed a corresponding member of the Academy of Sciences. Two years later he successfully demonstrated the reproduction of some forms of worms by simple fission; and in the same year he discovered the pores, or "stigmata," by which caterpillars and butterflies breathe, and made important studies in the structure of the tapeworm.

Turning to botany, and newly appointed a fellow of the Royal Society, the youthful scientist next experimented in plant physiology with special reference to the functions of leaves, and attempted to prove that all chlorophyllic plants are endowed with sensation and what he termed "discoverment." It was at this stage of his career that threatened blindness diverted his studies into an entirely different field.

De Bonnet's philosophical theories were largely influenced by the time in which he lived; he wrote a work on the "Proofs of Christianity" to defend Revelation, and valiantly opposed the teachings of Voltaire and Rousseau, and the epigenesis theory of Buffon. On the other hand, he advanced the purely materialistic idea that all thought is due to vibrations of the nerves. Bodily activity, he said, is a necessary condition of thought.

Following Cuvier and Leibnitz in the doctrine of original creation by a Deity, de Bonnet then premised a "germ" of perfecting evolution in every living thing. In his "Contemplation of Nature," he taught that all beings in nature form a graduated and unbroken scale from lowest to highest, with no gaps from the lowest atom of matter to "Archangels"; though the flaw in his perfectability theory appears when he denies that the highest of his heirarchy can ever exactly equal Deity itself. In "Philosophic Palingenesis," he elaborated this doctrine to show the survival not merely of man, but of all animals, and the perfecting of their faculties in the future state. Man, he said, is composed of a material body and an immaterial mind, resident in his brain; but he carries within himself the germ of a more attenuated body which will clothe his mind in the next stage after life on earth—a curious approximation to some of the teachings of modern Spiritualism. What he does not make clear is whether he expects each individual to carry within himself the germ of his own perfectability, or whether it is only races of men and kinds of animals that are perfected *en masse*.

De Bonnet's philosophy is chiefly interesting as a commentary on his scientific attainments. If he had died at twenty-five, he would have left his most valuable achievements already accomplished; but if, two hundred years ago, he had never been born, the world of science even to-day would have been a great deal the loser.

Maynard Shipley

SCIENTIFIC EVENTS

THE PRESERVATION OF NATURAL CONDITIONS

For three years the Ecological Society of America has had a committee composed of about twenty-five interested persons, investigating the question of preserving natural conditions for scientific study. The work to date has been concerned with (*a*) listing and describing preserved areas and areas desirable for reservation, (*b*) determining the policies governing existing reservations and the desirability of reserving natural areas within them, (*c*) collecting arguments in favor of preserves, (*d*) determining lines of research and education, scientific, artistic and historical which require or can make use of reservations, and (*e*) methods which have been successfully employed in securing reservations. The matter in hand includes a list of more than six hundred areas in United States and Canada which are preserved or are desirable for preservation. It is evident that some types of natural conditions are not represented and for some localities no areas have been brought to our attention. Persons having information regarding areas desirable for preservation or already preserved or knowledge concerning any of the subjects noted above, especially methods employed in securing reservations, are requested to send information, which will be fully credited, to the chairman or any member of the committee. The present committee is composed of C. W. Alvord (history), Univ. of Ill.; H. C. Cowles (plant communities), Univ. of Chicago; R. T. Fisher (forest practice), Harvard Univ.; S. A. Forbes (entomology), Univ. of Ill., A. S. Pearse (aquatic preserves), Univ. Wis., C. F. Korstian (grazing), Ogden, Utah; R. B. Miller (forest laws), Univ. of Ill.; T. C. Stephens (bird preserves), Sioux City, Ia.; R. H. Wolcott (fires), Univ. of Nebr.; F. B. Sumner, La Jolla, California; M. J. Elrod, Univ. of Mont.; F. J. Lewis, Univ. of Alberta; John Davidson, Univ. of Br. Columbia; G. B. Rigg, Univ. of Washington; F. Ramaley, Univ. of Colo.; G. A. Pearson, Flagstaff, Ariz.; G. W. Goldsmith, Univ. of Nebr.; J. R. Watson, Univ. of Fla.; J. W. Harshberger, Univ. of Pa.; W. L. Bray, Syra-

cuse Univ.; C. D. Howe, Univ. of Toronto; F. E. Lloyd, McGill Univ.; C. O. Rosendahl, Univ. of Minn.

V. E. SHELFORD, *Chairman*
UNIVERSITY OF ILLINOIS

THE NATIONAL COMMITTEE ON MATHEMATICAL REQIREMENTS

AT the last meeting of the General Education Board in New York on February 28, the sum of $25,000 was appropriated for the use of the National Committee on Mathematical Requirements to continue its work for the year beginning July 1, 1920.

A preliminary report on "The Reorganization of the First Courses in Secondary School Mathematics" was published for the Committee by the U. S. Bureau of Education about the middle of February. It has been distributed widely. Copies of the report have gone to all the state departments of education, to all county and district superintendents in the United States and to all city superintendents in cities and towns of over 2,500 population. It has been sent to all the normal schools in the country, to some 1,500 libraries and to almost 300 periodicals and newspapers. In addition it has been sent to about 4,500 individuals, the names and addresses of which were furnished the Bureau of Education by the National Committee. This list of individuals consists chiefly of teachers of mathematics and principals of schools throughout the country. Additions to this mailing list to secure future copies of the reports of the committee can still be made. Individuals interested in securing these reports should send their names and addresses to the chairman of the committee (J. W. Young, Hanover, N. H.).

A subcommittee consisting of Professor C. N. Moore, of the University of Cincinnati, Mr. W. F. Downey, of Boston, and Miss Eula Weeks, of St. Louis, has been appointed to prepare a report for the Committee on Elective Courses in Mathematics for Secondary Schools. Any material or suggestions for this report may be sent directly to the chairman of the subcommittee.

The recent work of the national committee had a place on the program of the organization meeting of the National Council of Teachers of Mathematics held in Cleveland on February 24 in connection with the meeting of the Department of Superintendence of the National Education Association. The meeting for the organization of the National Council was enthusiastically attended. A constitution was adopted and officers and an executive committee elected. Mr. J. A. Foberg, of the National Committee on Mathematical Requirements, was elected secretary-treasurer of the National Council.

Recent meetings of teachers at which the reports of the national committee have been discussed have taken place in New York City, Cincinnati, San Francisco, Cleveland, Oklahoma, Philadelphia, Springfield (Mass.), Providence (R. I.). Meetings in April will take place in Alabama, Illinois, Iowa, Michigan and Kentucky.

THE NEW YORK STATE COLLEGE OF AGRICULTURE AND THE NEW YORK STATE EXPERIMENT STATION

THE State College of Agriculture at Ithaca and the State Agricultural Experiment Station at Geneva have now become formally affiliated. Each will retain its separate organization and carry on its own appropriate work; in addition provision is made for somewhat closer correlation, for ready exchange of all facilities of research and experimentation, and for more frequent conferences. To these ends the trustees of Cornell University have appointed to the staff of the college eight persons on the staff of the station at Geneva: Whitman H. Jordan, director; R. J. Anderson, chemist; Robert S. Breed, bacteriologist; R. C. Collinson, chemist; U. P. Hendrick, horticulturist; Percival J. Parrott, '06, entomologist; Fred C. Stewart, '98, botanist; and L. L. Van Slyke, specialist in fertilizers. And reciprocally the board of control has appointed to the Geneva staff six members of the agricultural faculty: Professors Chandler, Emerson, Herrick, Lyon, Reddick, and Stocking.

The *Cornell Alumni Weekly* says: "This closer relationship promises benefits not only to the college, particularly in enlarging the

regular opportunities of graduate students and investigators, but also to the farming interests of the state, to whom the combined efforts and results are valuable. The affiliation, thus bringing a mutual extension of privileges, is characterized by the authorities as a gain to both institutions without cost or loss to either."

SCIENTIFIC NOTES AND NEWS

The next meeting of the American Astronomical Society will be held at Smith College Observatory, Northampton, Massachusetts, beginning on September 1. The society will also visit the observatory at Mt. Holyoke College.

The American Association of Anatomists will hold their annual meeting at the National Museum, Washington, D. C., from April 1 to 3. The program contains about sixty titles for papers and fifty demonstrations.

The second annual meeting of the American Society of Mammalogists will be held in the American Museum of Natural History, New York City, May 3–5, 1920. There will be opportunities to visit the New York Zoological Park, the Brooklyn Museum, the New York Aquarium, and other institutions of interest to members. Headquarters will be at the Hotel York, 7th Avenue and 36th Street, three blocks north of the Pennsylvania Station.

Dr. John Charles Hessler has been appointed assistant director of the Mellon Institute of Industrial Research of the University of Pittsburgh. Dr. Hessler, who is now serving as president of James Milliken University, Decatur, Ill., will enter upon his new work at the close of the present academic year. As a member of the administrative staff of the Mellon Institute, he will be in supervisory charge of certain of the researches in organic chemistry, a field in which he has specialized during the past twenty years.

Dr. John W. Macfarlane, professor of botany and director of the Botanical Laboratory and of the Botanic Gardens of the University of Pennsylvania, has tendered his resignation after twenty-eight years of service, to take effect on June 30.

Dr. Waldemar T. Schaller has resigned as chemist in the division of physical and chemical research, United States Geological Survey, and has accepted a position with the Great Southern Sulphur Co., Inc., of New Orleans, La., operating at Orla, Texas.

The French government has conferred the decoration, "Officier de l'Instruction Publique," upon Professor E. B. Van Vleck, of the department of mathematics of the University of Wisconsin, in recognition of his services as teacher and investigator and for his work during the war.

Professor Warren H. Lewis, of the Johns Hopkins Medical School, has been elected an honorary member of the Society of Medicine of Gand.

At its meeting held on March 10, the Rumford Committee of the American Academy of Arts and Sciences appropriated the sum of $250 to Professor Julius Stieglitz in aid of the publication of Marie's "Tables of Constants."

At a meeting of the Royal Society of the Medical and Natural Sciences of Brussels held on December 1, Dr. John J. Abel, professor of pharmacology at the Johns Hopkins University, was elected an associate member of the society.

The Committee on Scientific Research of the American Medical Association has made these grants for scientific work: Professor G. Carl Huber, University of Michigan, for study of nerve repair, $400. Professor H. M. Evans, University of California, for study of the influence of endocrine glands on ovulation, $400. Professor E. R. LeCount, Rush Medical College, for study of extradural hemorrhage and of the hydrogen-ion content of the blood in experimental streptococcus infections, $200. Dr. E. E. Ecker, Western Reserve University, for a study of the specificness of antianaphylaxis, $200. Dr. Henrietta Calhoun, Iowa, State University, for a study of the effect of protein shock on diphtheria intoxication, $400.

THE council of the Royal Society has recommended the following: Dr. Edward Frankland Armstrong, Sir Jagadis Chunder Bose, Dr. Robert Broom, Professor Edward Provan Cathcart, Mr. Alfred Chaston Chapman, Dr. Arthur Price Chattock, Mr. Arthur William Hill, Dr. Cargill Gilston Knott, Professor Frederick Alexander Lindemann, Dr. Francis Hugh Adam Marshall, Dr. Thomas Ralph Merton, Dr. Robert Cyril Layton Perkins, Professor Henry Crozier Plummer, Professor Robert Robinson, and Professor John William Watson Stephens.

AT the annual meeting of the Optical Society, London, Mr. R. S. Whipple was elected to the presidency; the vice-presidents are: Professor F. J. Cheshire, Sir Herbert Jackson, and Mr. H. F. Purser.

PROFESSOR B. A. HOUSSAY, of the University of Buenos Aires, has been elected corresponding member of the Société de Pathologie exotique at Paris in token of appreciation for his extensive research on snake venom and on scorpion and spider poisons.

DR. CHALMERS MITCHELL, the English zoologist, under the auspices of the London *Times*, undertook to make a flight from Cairo to the Cape with special reference to scientific observations, leaving Cairo in a Vickers-Vimy machine with a crew of four pilots and mechanics on February 6. A forced descent after delays by engine troubles at Tabora, in the Tanganyika territory damaged the machine so that the flight could not be continued.

MR. CARL L. HUBBS, assistant curator of ichthyology and herpetology in the Field Museum of Natural History, has resigned to accept the position of curator of fishes in the Museum of Zoology, University of Michigan.

ASSISTANT PROFESSOR GERALD L. WENDT, of the department of chemistry at the University of Chicago, has been appointed associate editor of the *Journal of the Radiological Society of North America*.

FRANK H. REED, Ph.D. (Chicago, '17), has been made supervisor of Industrial Research for the Butterworth-Judson Corporation of Newark, New Jersey.

DR. E. P. WIGHTMAN, recently of Parke Davis and Co., of Detroit, has accepted a position as research chemist with the Eastman Kodak Co., Rochester, N. Y.

LIEUTENANT SCHACHNE ISAACS, formerly instructor in psychology at the University of Cincinnati, and at present psychologist in the Air Service, Medical Research Laboratory, Mitchell Field, Long Island, has been awarded the fellowship in psychology offered by the Society for American Fellowships in French universities. This enables the holder to do graduate work in the French universities for two years. The purpose of the society is to develop an appreciation among American scholars of French achievements in science and learning.

DR. CHARLES R. STOCKARD, professor of anatomy at Cornell University Medical School, New York City, read a paper on "Growth Rate and its Influence on Structural Perfection and Mental Reactions" before the Philadelphia Psychiatric Society, on March 12.

A SPECIAL meeting of the College of Physisians of Philadelphia was held March 19, as a memorial to Dr. Horatio C. Wood. Dr. George E. de Schweinitz read a memoir to Dr. Wood. "Recollections of a Pioneer in Pharmacology in the United States," was read by Dr. Hobart A. Hare; "An Appreciation," by Dr. Francis X. Dercum, and "Reminiscences, Chiefly Neurological and Medico-Legal," by Dr. Charles K. Mills.

DR. GEORGE D. ALLEN, instructor in zoology in the University of Minnesota, died from pneumonia on March 11.

DR. K. A. J. MACKENZIE, dean of the medical department of the University of Oregon, a surgeon of national reputation, is dead at Portland, Ore., from heart disease superinduced by influenza.

UNIVERSITY AND EDUCATIONAL NEWS

THE University of Michigan has received an anonymous gift of one million dollars.

Rentals amounting to $2,367,000 will go to the university under the terms of a lease arranged by Levi L. Barbour, the Detroit manufacturer, with the stipulation that the money shall be used for educating women of the Far East.

Cornell University has received a gift of $100,000 for a new dormitory, to be named for the donors' parents, from W. G. Mennen and his sister, Mrs. Emma Mennon Williams, of Detroit.

Bates College is to receive $500,000 from the fund to be raised by the Northern Baptist Convention.

On recommendation of the medical faculty of Cornell University, women who are students in medicine may hereafter take the first year's work at the Medical College in New York City.

Professor Walter Edward McCourt, head of the department of geology of Washington University, has been appointed dean of the schools of engineering and architecture of Cornell University. He will assume the duties of his new position at once. The appointment was made to fill the vacancy caused by the resignation of Professor A. S. Langsdorf.

Professor E. T. Bartholomew, of the department of botany of the University of Wisconsin has accepted a research professorship in the Graduate School of Tropical Agriculture at Riverside, Cal., in connection with the University of California. His special work will be the investigation of the diseases of lemons and other citrus fruits.

Sir Archibald E. Garrod has been appointed to be regius professor of medicine in the University of Oxford in succession to the late Sir William Osler.

DISCUSSION AND CORRESPONDENCE

MODERN INTERPRETATION OF DIFFERENTIALS

To the Editor of Science: Without attempting to discuss the historical questions involved, I wish to point out that the theory of "differentials" given by Professor A. S. Hathaway in Science for February 13, 1920, would prove highly misleading to the modern student.

Professor Hathaway defines $\Delta' y$ as $N\Delta y$, where N is some multiplier and Δy a simple increment, and then defines dy as the limit of $\Delta' y$ as Δy approaches zero. The inevitable consequence of such a definition is that $dy = 0$, which is obviously futile.

In view of the continual recrudescence of such fallacies (with or without a historical background), it may be worth while to repeat here the modern interpretation of the differential, though this may be found correctly stated in any good text-book of calculus.

Consider the graph of a function $y = f(x)$, with the tangent line drawn at the point $x = x_1$, $y = y_1$. Give x an arbitrary increment

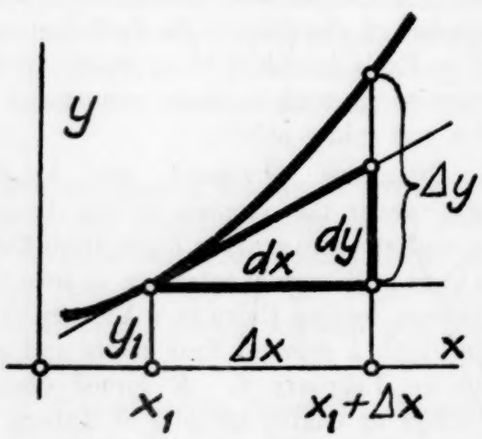

Fig. 1.

which, since x is the independent variable, may be denoted indifferently by Δx or dx. Corresponding to any such increment in x we have the *increment* of y, called Δy, extending up to the curve, and the *differential* of y, called dy, extending up to the tangent. Now when Δx (or dx) is made to approach zero, the ratio dy/dx remains constant, being the slope of the tangent line, while the ratio $\Delta y/\Delta x$ is a variable, approaching the slope of the tangent as a limit. *But the limit of Δy taken by itself is zero, and the limit of dy taken by itself is also zero.*

There are thus two very good reasons why

we can not say that "*dy* is the limit of Δy." First, *dy* is a variable and therefore can not be the limit of anything; secondly, zero is the limit of Δy, and therefore nothing else can be.

A list of similar fallacies, which still persist in some books (and, apparently, in some classrooms also), may be found in a paper by the present writer on "The proper use of the differential in calculus." [1]

The word *derivative* means, of course, the ratio dy/dx.

EDWARD V. HUNTINGTON
HARVARD UNIVERSITY

WEIGHT AND CENTRIPETAL ACCELERATION

TO THE EDITOR OF SCIENCE: Mr. Carl Hering's suggestion for a new form of dynamic compass[1] ought to be challenged before some one organizes a company to work the idea out on a commercial basis. The fact is, of course, that the change in weight which Mr. Hering refers to occurs only when the motion is *in a circle having its center in the earth's axis.* Mr. Hering's disk is a plane tangent to the earth's surface and motion in this plane does not, on the basis of Newtonian mechanics, affect the weight of a body. It is understood of course, that the disk is not forced to remain tangent to the earth as the earth rotates. This would complicate the situation by introducing the gyroscopic effect. If the disk is mounted in gimbals so that the earth in turning does not force a change in direction of the shaft there would, as stated above, be no tendency of the shaft to set itself parallel with the earth's axis.

The suggestion that the light disk with equal weights at extremities of a diameter would rotate in balance when in a north and south plane, but out of balance in an east and west plane is equally mistaken. Any change in the weight of a body on the basis of Newtonian mechanics must be due to an acceleration of the body, part of the gravitational force being used to produce the acceleration. We may, therefore, examine the accelerations of these bodies to see whether they could produce the effect described. Each of the weights on the light disk has an acceleration composed of two components.[2] One of these components is directed toward the center of the disk. This component is due to the rotation of the disk, and may be called the *disk component.* Since the two weights are at opposite extremities of a diameter the disk components of their acceleration are equal in magnitude and opposite in direction, and their only effect is to produce the well-known centrifugal stress in the disk. The other component of acceleration is common to the two weights. It is the acceleration of the center of the disk due to the earth's motion. It is altogether independent of the rotation of the disk. This acceleration will affect the weights of the two bodies, but the effect will be the same for both bodies in all positions of the disk, and cannot therefore, produce unbalanced rotation.

Curiously enough there is another cause that would produce a minute unbalance in a disk of the sort just considered when rotating in any vertical plane at any point on the earth's surface. When the line of the weights is in a horizontal position let the weight of each be represented by w. Then neglecting the weight of the disk and shaft the downward pressure on the bearings is $2w$. When the line of the weights has turned through 90° to a vertical position one of the bodies has approached the earth and consequently its weight is increased. The other has receded from the earth but its weight has decreased less than the other increased since the attraction varies as the inverse *square* of the distance. Consequently the pressure on the bearings is greater when it is horizontal. This would produce a minute effect of unbalance which, however, would be just as great when the disk rotates slowly as when it rotates at high speed.

BURT L. NEWKIRK
UNIVERSITY OF MINNESOTA

[1] Society for the Promotion of Engineering Education: *Bulletin*, Vol. 4, pp. 19–28, 1914, or *Proceedings*, Vol. 22, pp. 115–124, 1915.

[1] SCIENCE, Vol. LI., p. 46.

[2] Gimbal mounting is assumed again to eliminate gyroscopic effect.

THE SITUATION OF SCIENTIFIC MEN IN RUSSIA

To the Editor of Science: The information about Professor Pavlov conveyed in a letter to Science (March 12) is somewhat puzzling in its purport. It is customary to make announcement of events which actually occurred; as for instance birth, deaths, marriages, etc. It would be a most unique procedure to treat the public to news items like these: so-and-so has not yet been born, has not yet died, married, got an increase in salary. Why then this item that on a certain date A.D. Professor Pavlov *was not yet dead?*

It seems likely, therefore, that the only object of the note was to give publicity to a quotation from a letter of Pavlov to some other party to the effect that he was starving and instead of engaging in scientific pursuits was occupied in peeling potatoes. Now, this alleged quotation bears earmarks of a spurious nature. It undoubtedly belongs to that class of hoaxes which the daily press has been imposing upon its innocent readers with an invidious design. It is impossible to reconcile the two statements in the quotation, that Professor Pavlov is starving, and that he has so many potatoes to peel as to be obliged on that account to forsake his science. Even one not versed in the theory of nutrition would be skeptical about the probability of starvation in the midst of plenty of potatoes. (Consult Hinhede on the nutritional value of the potato.)

Like all statements intended primarily to force public opinion into a preformed mould, it is not what is actually said but what is indirectly implied that really matters. The quotation from Pavlov's letter is obviously calculated to rouse in us indignation over the sufferings of the distinguished physiologist. But does it not also insinuate a suggestion that the genius which was the man's great asset under the benign and enlightened government of the Czar of all the Russians has under the new régime become a crushing liability on him? So, ere we are moved to deep pity over Pavlov's unfortunate lot, let us reflect if with our well-meant sympathy we may not cause him more distress than comfort.

It so happens that I have some news of another venerable savant, Professor Timiriazev, distinguished botanist of the University of Moskow, an Sc.D. of Cambridge, a fellow of the Royal Society. As I have no "obvious" reason for hiding my informant, I may say that he is Arthur Ransome, whom I herewith quote:

> He [Timiriazev] is about eighty years old. His left arm is paralyzed, and, as he said, he can only work at his desk and not be out and about and help as he would wish. A venerable old savant, he was siting with a green dressing gown about him, for his little flat was very cold. He spoke of his old love for England and for the English people. Then speaking of the veil of lies drawn between Soviet Russia and the rest of the world, he broke down altogether and bent his head to hide his tears. I suffer doubly—he said—I suffer as a Russian, and, if I may say so, I suffer as an Englishman. My grandmother was actually English. I suffer as an Englishman when I see the country I love misled by lies, and I suffer as a Russian because those lies concern the country to which I belong, and the ideas which I am proud to hold.
>
> The old man rose with difficulty, for he, like every one else in Moskow, is half starved. "If I could let them know the truth—he said—those friends of mine in England, they would protest against actions which are unworthy of the England we have loved together."

S. Morgulis
The Creighton University

RUSSIAN AND AMERICAN SCIENTIFIC MEN

To the Editor of Science: In Science of March 5, I have noticed the report that Professor Pavlov, still alive in Petrograd last summer, was peeling potatoes when last heard from. Without wishing to jest on this truly pitiable situation, it may not be amiss to submit also the report that no small portion of the professors of this country are now likewise engaged in peeling potatoes or similar menial work, at any rate for a large part of their time. Under present conditions they can not get others to do such work for them.

The cause, here as in Russia, is the glorification of "labor"—apparently synonymous with cessation of labor, at any rate for a price proportioned to its value.

When a professor does not actually "quit his job," the public supposes he is giving the same service as formerly. In fact he may be simply meeting his classes as before, some ten or twenty hours in the week; the rest of his active time, which should be spent in preparation, study and research, is under present conditions too often dissipated in chores of house and garden, for which "help" is no more to be had. In effect the professor has "quit his job," for half time and in that half is situated somewhat like Professor Pavlov.

The irony of it is that the professor is the last man in the world to shirk his professional work, which is also his pleasure; but the topsy-turvy economics of the day are forcing many to do so.

A MEMBER OF THE EXPLOITED CLASSES

QUOTATIONS

NITROGEN FROM THE AIR AND THE BRITISH GOVERNMENT

THE report of the Nitrogen Products Committee has at last been allowed to emerge from the seclusion of the government pigeon-hole, in which it has reposed, in type, for at least seven months. It is a voluminous document of over 350 pages, containing the results of nearly three years' work, largely voluntary, on the part of a number of scientific men, who in that period explored in great detail the statistical and economic aspects of the nitrogen problems and also supervised much experimental research. The latter was devoted especially to the Haber process for the synthetic manufacture of ammonia by the direct union of its elements, nitrogen and hydrogen—a process which, coupled with the oxidation of the ammonia to nitric acid, undoubtedly enabled Germany, cut off from supplies of nitrate from Chile, to continue the war longer than would otherwise have been possible. The general principles of that process were familiar enough in this country, but acquaintance with the technique of its operation was confined to Germany. However, the committee made such progress towards remedying this deficiency that in their report they feel justified in recommending the immediate establishment of the process on a "commercial unit" scale in this country and its extension up to a minimum of 10,000 tons of ammonia annually.

For this purpose they suggest the utilization of a factory at Billingham-on-Tees. The Explosives Department of the Ministry of Munitions decided to start this factory in a hurry, and perhaps in advance of the technical knowledge available at the time, towards the end of 1917; but their attitude towards it was somewhat Laodicean, and it has not been finished. Its completion would cost a considerable sum, but the committee's view is that, as a matter of national insurance, we ought to be in a position to manufacture nitrates artificially in this country, since, from the military aspect, we cannot afford the risk of being dependent on saltpeter imported from Chile for the nitrogen compounds which are indispensable for modern high explosives. Perhaps the best solution would be for private enterprise to take over and equip the factory, with some measure of government control and interest; and the appearance a few weeks ago of an advertisement inviting offers for it suggests that this is the direction in which events are moving. It is believed, indeed, that an important group of firms is in negotiation for the place. In this connection it must be remembered that nitrates are as essential in peace, for fertilizing purposes and the manufacture of mining explosives, as they are in war.

A cheap and abundant supply of electric power being essential for the commercial success of some of the processes of fixing atmospheric nitrogen, the committee considered very fully the question whether this condition can be met in the United Kingdom. In particular, they investigated the possible advantages of employing preliminary processes of carbonization and gasification in connection with large electric power stations, instead of firing

the coal direct into the furnaces of steam boilers. Such methods offer the attraction that they permit the recovery of by-products that are lost with direct firing, and it is, therefore, disappointing to find that the committee's conclusions are adverse. They conclude that, in the present state of knowledge, the direct burning of coal under steam boilers forms the cheapest method of generating electricity on a large scale from coal, even when the indirect processes are credited with the revenue obtainable from the sale of the recovered by-products. What is still more unfortunate—from the point of view of those who hope for an increased supply of home-produced liquid fuel, as well as cheaper electricity from capital power stations with gas-fired boilers—they make out that the advantage of direct firing increases with rising costs of coal and labor.—The London *Times*.

NOTES ON METEOROLOGY AND CLIMATOLOGY

RAINFALL (AND SNOWFALL) OF THE UNITED STATES[1]

THE Weather Bureau has just issued a reprint from the *Monthly Weather Review* entitled "Seasonal distribution of precipitation and its frequency and intensity in the United States,"[2] by Joseph B. Kincer. Three reviews and abstracts are included in the reprint: "Some characteristics of the rainfall of the United States,"[3] by R. DeC. Ward; "New seasonal precipitation factor of interest to geographers and agriculturalists,"[4] by R. M. Harper; and "The snowfall of the United States,"[5] by R. DeC. Ward. Since these three papers are easily available, this note will cover only Mr. Kincer's article and the graphs added to the reviews of Professor Ward's two papers.

Here are published, for the first time, reliable and detailed maps of the average rainfall of the whole United States for each month. The topographic (hachured) base-map used shows at once the close dependence of rainfall on topography as it affects precipitation of moisture from the prevailing westerly winds. We have long known of the marked spring and early summer rainfall maximum in the prairies and Great Plains; but these monthly maps give us almost a moving picture of the wave of rainfall which spreads northward and westward as the warm southerly winds blow in day after day from the Gulf of Mexico. From its February position across east Texas, northwest Arkansas and southern Illinois, the 3-inch monthly rainfall line in March has moved westward into Oklahoma, central Missouri and northern Illinois; in April, to central Texas, central Oklahoma, eastern Kansas and central Iowa; in May, to the 101st meridian in south Texas, across the Panhandle into northeastern New Mexico, through western Kansas, west central Nebraska, the Dakotas and northern Minnesota, and in June still farther westward in the central and northern Great Plains—in Montana even to the Rockies. By June in the southern Plains and by July in the northren Plains the spring-time flood of moist air has spent itself, and the rainfall lines are beginning to retreat—eastward as the summer passes, and southward as the coldness of the oncoming winter renders much precipitation impossible. The four maps of precipitation by seasons summarize this same movement of the isohyets. With such a series of maps before one it is obvious that the Gulf of Mexico and the open country to the north and northwest allow our prairies and plains to be so productive.

If the conditions year after year were like those shown on these maps of average rainfall, we should not have been experiencing or

[1] *Cf.* notes on this subject in SCIENCE, July 19, 1918, N. S., Vol. XLVIII., pp. 69–72 (snow, SCIENCE, February 11, 1916, N. S., Vol. XLIII., pp. 212–214).

[2] September and October, 1919, Vol. 47, pp. 624–633, 695–696, 7 graphs, 30 maps—13 in text and 17 full-page lithographs. (For copies, apply to "Chief, U. S. Weather Bureau, Washington, D. C.")

[3] *Scientific Monthly*, September, 1919, Vol. 9, pp. 210–223.

[4] SCIENCE, August 30, 1918, N. S., Vol. XLVIII., pp. 208–211.

[5] *Scientific Monthly*, November, 1919, Vol. 9, pp. 397–415, map.

reading of the great droughts, recently ended, which were at their worst in west Texas and the northern Great Plains. The flood of warm, moist air from the Gulf is variable in size and duration. These variations are felt most near its western and northwestern limits, where farmers have learned to look on partial crop failures as normal. This variability, which is the most important aspect of rainfall, aside from the average amount, is clearly brought out by Mr. Kincer in a number of graphs and maps. In drought years as well as in years of plenty, farmers are inclined to believe in stories of progressively decreasing or increasing rainfall: comparisons of rainfall averages by successive 20-year periods show, however, that in this region there is no perceptible progressive change in rainfall.

In years of decreasing rainfall, real-estate agents for the semi-arid lands of western Kansas explain to prospective buyers that although the total rainfall is decreasing, the decrease is mostly confined to the washing and flooding downpours, and that the proportion of rains of beneficial amount is increasing. They are discussing another essential element which must be considered in comprehensive rainfall discussions. Mr. Kincer presents maps showing the average annual number of days with precipitation 0.01 to 0.25 inch, 0.26 to 1.00 inch, and 2.00 inches or more. Further details of rainfall intensity are given on maps showing the average annual number of days with precipitation more than 1.00 inch in an hour, and the maximum precipitation in 24 hours. Two more maps which might be called "drouth maps" show the percentage of years with 30 consecutive days or more without 0.25 inch of rainfall in twenty-four hours from March 1 to September 30, and the greatest number of consecutive days without 0.25 inch of rainfall from March 1 to September 30. These are all based on the rainfall data for the 20-year period, 1895–1914.

There are three snow maps presented. A large one shows the average annual snowfall of the United States, 1895–1914, drawn on a topographic base-map with close attention to the effects of altitude and exposure. The other two maps show the average annual number of days (1) with measurable snowfall, and (2) with snow cover. In the eastern United States (except near the Atlantic) the line of one day with snow cover (the average of several days in one winter, with no days in several years) is near the 33d parallel of latitude; that of 30 days with snow cover lies close to the 39th parallel; that of 60 days near the 42d; that of 90 days near the 43d, and that of 120 from near the 44th in the East to the 47th in Minnesota. As a broad generalization, the number of days with snowfall is about half the number of days with snow cover.

The publication of these interesting precipitation maps with the discussion makes us hope that still another year will not pass before the issue of the long-expected precipitation section of the Atlas of American Agriculture, with its colored maps, carefully made graphs and detailed discussion. Still later, the folio on temperature and the other climatic elements are to come.

CHARLES F. BROOKS

WASHINGTON, D. C.

SPECIAL ARTICLES

INTERSEXES IN DROSOPHILA SIMULANS

ON the first day of January, 1920, a stock of *Drosophila simulans* Sturtevant[1] from Rochester, Minn., was found to contain intersexual individuals. Over 200 such intersexual specimens from this stock and derivatives of it have now been examined. About a dozen of them have been dissected and about the same number have been cleared in KOH and examined in balsam. All these specimens apparently belong to a single type. Male and female parts are both present, as will appear from the following table.

The intersexes are sterile, inasmuch as their gonads are almost, if not quite, absent. Their sexual behavior seems to agree best with that of the normal females. They are courted by males, but mating has not been seen.

[1] For a description of this species see *Psyche* (1919), 26, p. 153.

	Males	Females	Intersexes
Sex combs on fore tarsi	Present	Absent	Absent
Number of dorsal abdominal tergites	5	7	7
Ovipositor	Absent	Present	Present
Spermathecæ	None	2	2
Penis	Present	Absent	Absent
First genital tergite	Present	Absent	Present
Anal plates	Lateral	Dorsal and ventral	Lateral
Claspers	Present	Absent	Present
Tip of abdomen	Black	Banded	Black
Gonads	Testes	Ovaries	Very minute if present

Crosses of normals from the intersex stock have made it possible to study the character. The intersexes are modified females—*i. e.*, they have two X-chromosomes. This is shown by the fact that in cultures in which half of the males show sex-linked recessive characters but all the females are wild-type, the intersexes never show these sex-linked characters. This relation has been found to hold true for three sex-linked characters that are not closely linked to each other; and the intersex gene itself has been found not to be sex-linked (see below). Therefore the relation just noted can not be due to linkage between the intersex gene and the sex-linked genes in question.

Numerous crosses of the intersex stock to unrelated stocks have never given intersexes in F_1, but have frequently produced them in F_2. The intersex character is therefore recessive.

Pair matings that have produced intersexes have given a total of 510♀: 165 intersex: 754♂. There is an excess of males, but this is evidently a 3:1 ratio of females to intersexes, indicating not only that the gene is recessive but also that it is not sex-linked. The final proof of the latter point has been obtained through the discovery that the intersex gene is linked to the autosomal recessive gene for "plum" eye-color. Three F_1 pairs from a mating between the intersex stock and the plum stock have given in F_2:

Females		Intersexes		Males	
Wild-type	Plum	Wild-type	Plum	Wild type	Plum
198	91	87	0	293	65

The absence of the intersex plum class shows that the two genes are linked; and plum is known to be an autosomal recessive.

It has been shown by Morgan and Bridges[2] that individuals of *D. melanogaster*[3] that are partly male and partly female are produced, though only rarely, by most stocks. These "gynandromorphs" have been shown, by genetic evidence, to have two X-chromosomes in their female parts and only one X in their male parts. They are sex mosaics, and each part develops as it would in a whole animal of the same genetic constitution. There is strong evidence that the intersexes described here are not of this nature. The male and female parts in them probably both possess two X-chromosomes. This has been shown as follows. A total of 104 intersexes have been produced by females heterozygous for the sex-linked gene for "yellow" hairs and bristles. Half of these intersexes—about 50—must then themselves have been heterozygous for yellow. If the intersexes are really gynandromorphs, the male parts at the posterior end of the abdomen should have contained a single X-chromosome, and in about half of the specimens that were heterozygous for yellow (*i. e.*, in about 25 individuals) this should have been the yellow-bearing X. As Morgan and Bridges have shown, these parts should then have borne yellow hairs and bristles. The 104 intersexes were all carefully examined for this point, and *none of them had yellow male parts.*

We may conclude that the intersexes are females, modified by a recessive autosomal mutant gene that causes them to show male parts, though these parts themselves still have two X-chromosomes. The normal sex-determining mechanism is not affected at all, but the end result is modified by a gene that is not even in the sex chromosomes. It has

[2] Carnegie Inst. Washington (1919), publ. 278, pp. 3–122.

[3] I have unpublished data on exactly similar cases in *D. simulans* itself.

been assumed by Goldschmidt,[4] Hertwig,[5] Banta,[6] and others working with intersexes that in their animals the normal sex-determining mechanism itself was failing to function as usual. The present example shows that such an assumption can not be accepted without proof.

A. H. STURTEVANT
COLUMBIA UNIVERSITY AND
CARNEGIE INSTITUTION

THE ILLINOIS STATE ACADEMY OF SCIENCE

THE thirteenth annual meeting of the Illinois State Academy of Science was held at Danville, Illinois, February 20 and 21, 1920, under the presidency of Dr. Henry B. Ward, of the University of Illinois.

The principal items of business transacted were the following: The academy voted unanimously to become affiliated with the American Association for the Advancement of Science under the terms adopted by the council of the association at the St. Louis meeting. It was voted that one half-day session of the next annual meeting be devoted to section meetings and the following sections were provided for: medicine and public health; biology and agriculture; geology and geography; chemistry and physics; mathematics and allied sciences; the science of education and education in science. It was voted that the council of the academy be empowered to select chairmen for these sections. The committee appointed last year to secure affiliation of science clubs in high schools with the academy reported five such clubs which had accepted the terms of affiliation, two of these taking national membership under the plan of affiliation with the American Association for the Advancement of Science.

In addition to the regular program of scientific papers, Dr. Henry B. Ward, president of the academy delivered an illustrated lecture on Alaska.

The following officers were elected for the ensuing year: Dr. Henry C. Cowles, University of Chicago, president; Dr. Chas T. Knipp, University of Illinois, vice-president; J. L. Pricer, State Normal University, Normal, secretary; Dr. W. G. Waterman, Northwestern University, treasurer. Dr. A. R. Crook, State Museum, Springfield, is ex-officio librarian of the academy, in charge of the sale of back numbers of the transactions and of the exchange of current issues.

One hundred and five new members were elected to the academy.

The following are the titles of the papers presented at the different sessions:

Development of smokeless fuel from Illinois coal: PROFESSOR S. W. PARR, University of Illinois, Urbana.

Tastes and odors in the Danville water supply in the summer of 1919: DR. EDWARD BARTOW and R. E. GREENFIELD, Illinois State Water Survey, Urbana, and H. N. ELY, Superintendent, Interstate Water Co., Danville.

A new test indicator for water analysis: R. E. GREENFIELD, Illinois State Water Survey, Urbana.

The founding of sanitary districts: DR. EDWARD BARTOW, Illinois State Water Survey, Urbana.

Some comments on the present status of tuberculosis: DR. WALTER G. BAIN, St. John's Hospital, Springfield. During the war, chief of the laboratory service of the U. S. Army General Hospital No. 8.

Statistical study of the incidence and mortality of influenza in Illinois: DR. HENRY B. HEMENWAY, Division of Vital Statistics, State Department of Public Health, Springfield.

Report of progress at Illinois State Museum: DR. A. R. CROOK, chief of Division of State Museum, Springfield.

Gaining and losing power: C. L. REDFIELD, Chicago.

The progress of barberry eradication in Illinois: L. R. TEHON, assistant pathologist, U. S. Department of Agriculture.

Road oil and its uses: DR. A. F. GILMAN, Illinois Wesleyan University, Bloomington.

The absorbtion of oxides of nitrogen formed in silent discharge: DR. F. O. ANDEREGG, Purdue University, Lafayette, Ind.

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